independent of prior conditions, in which case it is removed from the sphere of law altogether, and becomes miraculous; or it may mean a distinct tendency to variation inherent in the offspring, and impressed upon it by the parent. In the latter case, however, spontaneity is really hereditary; and only appears to be spontaneous because it is the disclosure of a previously hidden power. M. Ribot fails, so far as I can find, to discriminate these meanings. He rejects the notion of spontaneity as wholly unscientific, but does not observe that the original life-germ must have contained inexplicable powers enabling it to develop into many forms. The seven hundred or more crystalline forms in which calcite is said to be found, must be explained partly by the intimate constitution of a molecule of carbonate of lime, partly by the environment in which it became crystallised. So we must attribute the almost infinitely varied forms of animal life partly to environment, but partly to the inexplicable powers of development impressed upon certain particles of protoplasm.

M. Ribot's reasoning is of doubtful soundness, again, when he speaks of heredity as the *cause* of decline in nations, or the cause of the production of new instincts. So far as the child is like its ancestors, there cannot on the average be either progress or decline. If certain individuals have, from unexplained causes, deviated from the previous type, it is impossible that their offspring should resemble completely both the previous and the new type. The contradictory features of different ancestors cannot possibly be made manifest in the same child; therefore the law of heredity must appear to fail in one way or the other. When a superior race intermarries with an inferior one, and becomes degraded, heredity simply perpetuates the inferior type by what Mr. Darwin calls *prepotency*; a term, by the bye, which M. Ribot should have adopted.

It cannot be said that M. Ribot is alone responsible for the want of consistency in his views of heredity. There are still some who believe in spontaneous generation; there are others who would have us believe that ordinary chemical agencies have developed a lifeless particle of protoplasm into a living particle, which became the germ of the animal and vegetable kingdoms. Mr. Darwin, so far as I remember, nowhere goes back to such insoluble questions. Sir W. Thomson suggests that the germ came from other parts of space. How far Mr. Herbert Spencer's philosophy affords a real solution of the question it must probably remain for another generation to decide. All that I wish to point out is, that so highly intelligent and careful a student of all that has been written on the philosophy of evolution as M. Ribot has certainly failed to acquire clear notions concerning the relations of heredity, spontaneity, and the influence of environment.

The most important result of M. Ribot's arguments is perhaps the support which he brings to Mr. Spencer's views of the origin of moral sentiments and rules. The last few chapters in which he treats of the moral consequences of heredity are particularly interesting. It becomes evidently impossible to uphold any longer the views of the older utilitarians, from Locke down to the two Mills and Buckle. As M. Ribot remarks, it is surprising to find a writer such as Buckle attributing little importance to psychological heredity. It is impossible any longer to look upon the mind and moral nature of the child as a *tabula rasa*, which can be marked by education at our will. If so, Mill's views of the philosophy of morals fall to the ground, and the doctrine of the moral sense in a modified form must be again taken in hand.

As a general rule, M. Ribot appears to acknowledge with sufficient candour his indebtedness to various authors. An exception is to be found in the case of Mr. Galton. It is true that Mr. Galton is quoted from time to time, but sometimes in a slighting manner; whereas the extensive obligations under which M. Ribot lies towards Mr. Galton will be apparent to anyone who is acquainted with the work on "Hereditary Genius" of the latter author.

W. STANLEY JEVONS

OUR BOOK SHELF

Animal Physiology. By John Cleland, M.D., F.R.S. Advanced Science Series. (Wm. Collins, Sons, and Co.)

HUMAN Physiology being in a great measure based upon investigations conducted on the lower Vertebrata, all works on the subject may, in a certain sense, be considered to be on "animal" physiology. The small treatise before us agrees, as far as the nature of the points treated of, very much with most works of the same size on human physiology. Incidental mention is no doubt made of the most important peculiarities of the nervous, circulatory, digestive, and other systems in the lower Vertebrata, but these are incomplete, and sometimes inaccurate. As an introduction to physiology, Dr. Cleland's work, however, possesses many advantages. It is written for readers previously unacquainted with anatomical details, and this class of students is daily becoming more numerous, although it is generally felt that no considerable progress can ever be made in the subject except on an anatomical basis. The illustrations are also numerous, whilst many are original and excellent. The manner of expression is particularly simple and clear, all the technical terms employed being carefully explained. In the earlier part of the work, in the chapter on alimentation, there is an argument on which particular stress is laid, which is, that as animals have no power of manufacturing organic matter from the materials found in organic nature, but feed either directly on the vegetable world or on other animals which have fed on vegetables; and as in plants the power of building organic matter is confined to the green parts, "the statement may therefore be ventured on that, so far as observation has yet proceeded, it would appear that the presence of chlorophyll is as necessary for the production of organic matter in organisms as the presence of protoplasm is necessary for growth." The full bearing of this fact is, no doubt, not yet fully understood. On the whole, we think that the author has fully succeeded in producing a work which, from the grouping of its facts, is decidedly more than a mere collection of details.

Fifth Annual Report of the Association for the Improvement of Geometrical Teaching. (January 1875.)

THE Association, it may be remarked, is almost coeval with this journal, for it was in the early numbers of NATURE that a correspondence was started on the subject of Geometrical Teaching. This resulted, as our readers are aware, in the formation of the Association. After four years of continuous work, two of which have been devoted to the difficult subject of Proportion (as we learn from the Report), the Syllabus of Plane Geometry is now complete; and, after a few verbal alterations

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possibly have been made, it will be forwarded for criticism to the Committee (on Geometrical Teaching) of the British Association and to other mathematical authorities. The object, we further learn, is, if possible, to get the sanction of the British Association; and this backing the opinion of the large number of mathematical teachers who now form the Association, will, it is hoped, lead the examining bodies of the country to act with perfect impartiality in considering the merits of those pupils who have been trained in accordance with the methods of the Syllabus as contrasted with the favourers of Euclid.

From the Report we gather that the principal work of the Association is expected to be completed in another two years; it is not attempted to forecast what will be its subsequent work. Perhaps, as has, we believe, been suggested, it may become an Association for the Improvement of Mathematical Teaching.

As the publications of the Association are for private circulation, we cannot go into further detail; we may, however, say that it has done good work in having been the moving cause of five valuable Presidential Addresses.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Influence of Pigments on the Photographic Image of the Spectrum

WHEN, some time since, Prof. H. Vogel announced the discovery that the addition of a pigment to a film of bromide of silver made it sensitive to light of the colour which that pigment gave it, though it had not been so previously, many—indeed I might say most—photographic chemists doubted the accuracy of his observations and the existence of any such law. His experiments were rehearsed by most of them, and the reports were, in almost every case, contradictory of his conclusions. There were powerful *à priori* reasons for doubting, amongst which the chief was, in my own opinion, that if a film coloured (say) red were sensitive to red light, it could not be developed under red light, but would fog, and would therefore be unworkable, which was not found to be the case. Another was, that the use of tinted films, well known for a long time, had only resulted in an universal retardation of all colours. It was, moreover, contrary to the known analogies of actinism that a purely mechanical admixture irrespective of any chemical quality should produce changes of so purely chemical a nature as those which are the basis of photographic action.

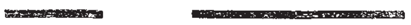
By the kindness of Mr. Lockyer I was enabled to experiment at his laboratory at South Kensington with the same plates (Col. Wortley's tinted films) that Prof. Vogel had based his discovery on, and, as I expected, found the results quite other than those the professor had announced. Although a protracted exposure (seventeen minutes) was given, and the more refrangible lines were quite buried by halation, no line was shown which did not appear in the ordinary wet collodion film.

That careful and excellent photographic chemist, Mr. Spiller, President of the London Photographic Society, Dr. Van Monckhoven, Mr. Carey Lea, and numerous others, amongst whom I am enabled, by his personal assurance, to name Dr. J. W. Draper, unquestionably the first living authority on spectrum photography, as well as his not less well-known son, Prof. Draper, have also followed Vogel in his experiments without obtaining any confirmation of his law.

Up to this time the only testimony confirmatory of his views offered is that of Becquerel, who, as the most marked instance of success, gives this—that chlorophyl (a green substance) gives great sensitiveness to *red* rays! That most indefatigable and precise experimentalist, Mr. Carey Lea (of Philadelphia, U. S. A.), in the course of a long series of experiments, unfortunately interrupted by his ill-health, showed that while coralline in a film did add slightly to the length of the spectrum image, other red pigments produced no effect whatever, and that salicine, which has no colour, produced more effect than coralline. But if chlorophyl, a green substance, is sensitive to red light, aniline green, so far as my own experiments go, produces no effect whatever except prolongation of the exposure necessary.

Now, without in the least disputing the prolongation of the spectrum photograph as claimed by Prof. Vogel, or depreciating the importance of his results, it seems to me that we are in a position to assume that he is entirely mistaken in the nature of the law he deduces, and that these results are due to purely chemical causes, in no wise dependent on colour, though in a few cases the colour may coincide with the chemical cause in such a way as to afford apparent confirmation of his hypothesis.

It must be remembered that Dr. Draper has long ago shown that all the rays have chemical activity, and that he has, without any such aid as Vogel has called in, produced complete photographic spectra; and has also shown that different substances decompose under different rays. Becquerel's experience with chlorophyl gives a clue to the connection between these discoveries and Vogel's results, if collated with a series of phenomena resumed by Dr. Draper (from observations by Dr. Gardner) in the interesting papers by him on the "Distribution of Chemical Force in the Spectrum"*:—"In Dr. Gardner's paper there are also some interesting facts respecting the bleaching or decolorisation of chlorophyl by light. He used an ethereal solution of that substance:—'The first action of light is perceived in the mean red rays. and it attains a maximum incomparably greater at that point than elsewhere. The next part affected is the indigo, and accompanying it there is an action from + 10·5 to + 36·0 of the same scale (Herschel's), beginning abruptly in Fraunhofer's blue. So striking is this whole result, that some of my earlier spectra contained a perfectly neutral space from − 5·0 to + 20·5, in which the chlorophyl was in no way changed, whilst the solar picture in the red was sharp and of a dazzling white. The maximum in the indigo was also bleached, producing a linear spectrum as follows:—



in which the orange, yellow, and green rays are neutral. These, it will be remembered, are active in forming chlorophyl.' . . . I have quoted these results in detail, because they illustrate in a striking manner the law that *vegetable colours are destroyed by rays complementary to those that have produced them*, and furnish proof that rays of every refrangibility may be chemically active." (P. 7, "Researches in Actinic Chemistry.")

Dr. Draper goes on in this memoir to establish a second proposition to this effect: "That the ray effective in producing chemical or molecular changes in any special substance is determined by the absorptive property of that substance." This proposition, laid down in 1841, seems to me to contain the explanation of all the phenomena of chemical or molecular change in photographic films; and if I might be permitted to offer an hypothesis supplementary to the proposition, serving, *if demonstrable*, as corollary to it, it would be that if two substances having different absorptive properties are simultaneously (or nearly so) subjected to the action of white light, in molecular contact the change in one of them may be communicated to the other mechanically. Thus, bromide of silver, which is not sensitive to the red ray, being placed in contact with chlorophyl, which *is* sensitive to that colour, the action of the red ray is communicated from the latter to the former substance, producing what may be designated as a sympathetic molecular effect. But in order that this may obtain, it is necessary that the auxiliary substance applied to influence the sensitive photographic film should be in itself sensitive to other rays than those which decompose the silver bromide. This would account for the effect of chlorophyl and perhaps for the original experiment which attracted the attention of Prof. Vogel, as the dry plates of Col. Wortley with which it was made contain salicine in their preservative as well as an aniline red in their substance, and Mr. Carey Lea has shown that salicine has the effect which Vogel claims for the colour.

If this is tenable, it follows that the object of our researches should be to discover those substances which have an independent susceptibility to actinic action, but for different rays than those which form the basis of the film experimented on. The results so far obtained in this direction, even those of Vogel himself, are, it seems to me, quite as capable of explanation by the hypothesis I have offered as by that of an arbitrary effect of colour; in confirmation of which we have only experiments (thus far made public) by Prof. Vogel himself.

It seems to me incredible that, if such a law existed, such

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* "Researches in Actinic Chemistry, Memoir Second," &c. John William Draper, M.D., LL.D., New York.

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